Response to RC1

Authors' responses are in red italics

Summary

To characterize the environments in which convective cells in cold frontal regions form, the manuscript combines convective cell tracking and detection with automatic front detection. The thermodynamic conditions and different lifting mechanisms for (i) convective cells, (ii) cell environments, (iii) and non-cell environments are compared using ERA5 data. Moreover, the results are binned in different categories based on their distance to the cold front location which facilitates to discriminate between post-frontal, frontal, and pre-frontal convection. Their results indicate that environmental conditions favorable for pre-surfacefrontal and post-frontal convection differ. For example, pre-surface frontal convection appears to be favored by large CAPE values, while for post-frontal cells mid-level relative humidity appears to be relevant for convection to be present. The manuscript addresses open questions concerning the initiation of convection near cold fronts, and the applied methods are suitable to address the research questions. Generally, the manuscript is well-structured and the figures are presented clearly. The research questions are clearly stated and of relevance for the readership of *Weather and Climate Dynamics*. I recommend publication of the manuscript in *Weather and Climate Dynamics* but I have several comments that should be addressed prior to publication, which are outlined in detail below.

We thank the reviewer for taking the time to review the manuscript and for their very useful and detailed comments. We are glad that the reviewer supports the publishing of this article in WCD subject to revisions.

1 General comments

1 General structure of the manuscript

Overall, the manuscript is well-structured. Yet, the Results Chapters include individual phrases and paragraphs that rather belong in the Introduction or Methods or Discussion section. I would ask the authors to make sure that the information is consistently placed in the appropriate sections and revise/streamline the Results Chapter accordingly. This does not pertain to the overall structure of the manuscript (which I think is good) but rather to several individual sentences/paragraphs. For example, Section 3.4 starts with one paragraph of introduction to Q-vector convergence which could be substantially shortened by moving the content to the Introduction and/or Methods Sections. Similarly, I would ask the authors to make sure that their discussion of results is consistently placed in the Discussion Section (and not in the Results Section).

We thank the reviewer for their constructive feedback regarding the structure of the manuscript. Regarding Section 3.4, since large-scale lifting is

not only relevant for the ascending motion itself but also increasing instability, we find it relevant to briefly emphasise this aspect in the results as it is key to interpreting the results. Nevertheless, we agree that these introductions to variables can be substantially shortened, and some points can be moved to other sections. This will be changed in the revised manuscript.

Regarding the discussion, the discussion section serves a specific purpose in this study to frame the results in the context of Pacey et al. (2023). Some discussion regarding the findings relative to other literature (not from Pacey et al. 2023) is therefore included in the results section. We will rename the current discussion section to "Relating the results to cold-frontal cell climatology"" to highlight that this is not a discussion in the traditional sense.

2 Introduction of data set

The presented study follows up on a study published by Pacey et al. (2023). I think the reader would profit from summarizing the key results from the predecessor study (Pacey et al., 2023) in more detail.

On L56–60 the results of Pacey et al. (2023) which are relevant for this study are already summarised. The confusion may arise from our use of "For example" on L56 which we suggest removing in the revised manuscript since the results relevant for the current study are already stated.

3 Quantification of uncertainty

Most figures show only mean values binned in distances from the front. I would appreciate if a measure of uncertainy (e.g., standard deviation as shading, percentiles, etc.) could be additionally shown to illustrate the associated uncertainty and variability. I appreciate that significant differences are emphasized in the figures, however, including a measure of uncertainty would be beneficial, for example, as shown by the boxplots in Fig. 5. Figure 5 also shows that the distributions are not Gaussian, i.e., it may be useful to show the median in addition to the mean for some variables that are not normally distributed.

We thank the reviewer for the nice suggestions. Uncertainty shadings are not shown due to the overlap of distributions which would make the plots overcrowded and hard to interpret. We show an example of the 25th and 75th percentiles shown by horizontal lines for the vertical velocity below (Figure R1) as well as an example with the median shown by a triangle (Figure R2). We opt to only add the median to each plot in the main paper since this allows better interpretability and still highlights whether a distribution is non-Gaussian and whether it is positively or negatively skewed.
Nevertheless, we will include discussion in the manuscript that some of the distributions overlap. Furthermore, the plots with horizontal lines for the 25th and 75th quartiles will be included in the supplementary material.

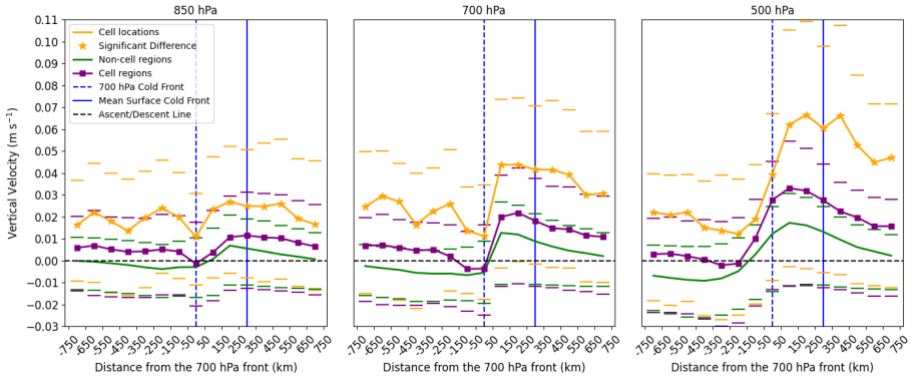

Figure R1: Vertical velocity at 850, 700 and 500 hPa depending on distance from the 700 hPa front (km) for cell grid points (orange), cell region grid points (purple) and non-cell region grid points (green). Positive and negative values indicate ascending and descending motion respectively. Horizontal lines indicate the 25th and 75th percentiles of the distributions.

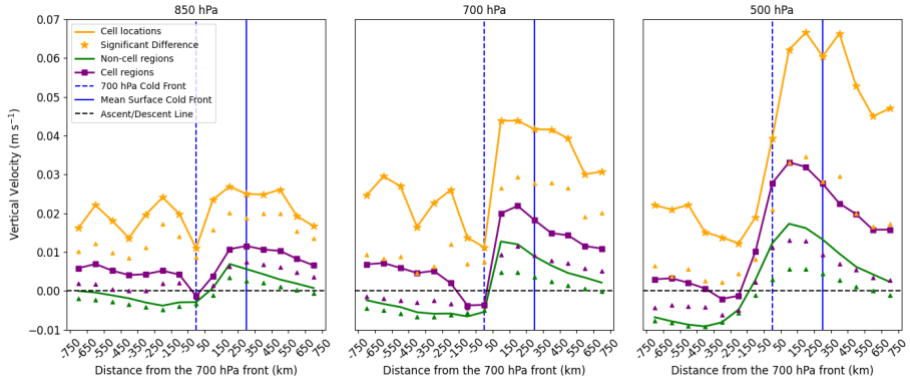

Figure R2: As Figure R1 but triangles indicate the median of the distributions.

4 Definitions of CIN (vs CAPE)

The authors apply only one (of many definitions) for CAPE but several definitions of CIN. I would ask the authors to better justify why different definitions are used for CIN (but not CAPE). I'm not sure the manuscript profits substantially from the other definitions of CIN (i.e., the authors could consider moving panels (b) and (c) of Fig. 5 to the Appendix to streamline the manuscript), as the key conclusions do not depend on the detailed definition of CIN. Instead, I would be interested to see a comparison of the CIN distribution of convective grid points with the CIN distribution of the convective environment (and non-convective

environment). Yet, the authors have mentioned that the computation of CIN for that many grid points is computationally demanding, and I understand if this is not feasible here.

The only CAPE available on the Climate Data Store is the MUCAPE and is available for all grid points in our study thus allowing a comparison between cell grid points, cell regions and non-cell regions (Figure 4). On the other hand, the CIN on the Climate Data Store assigns a missing value if CIN exceeds 1000 J kg^{-1} or where there is no cloud base. For the data points considered in our study 86% have a missing value and it is not clear how to deal with these. For this reason, we requested a different dataset (as discussed in Section 2.4.1) but only for cell grid points. Since this dataset also includes CIN considering different departure levels we decided to make use of the full dataset.

We thank the reviewer for the nice suggestion to move panels (b) and (c) of Fig. 5 to the appendix. We agree that this would allow greater consistency with the CAPE plot in Figure 4. We will also move Figure 10a to the appendix in the wind shear section for the same reason.

5 Conclusions

In the conclusions, the authors summarize the most relevant factors for convective initiation by discussing each variable separately (e.g., CAPE, RH, Q-vector convergence, etc.) following the structure in the Results Section. I would find it more helpful if the authors could cluster the key factors and differences for pre-surface-frontal, near-700hPa-frontal and post-700frontal as substantial differences between these categories are presented (i.e., similar to the structure in the Discussion Section). Finally, the authors have clearly defined three research questions in the introduction, which could be addressed again in the conclusions.

We thank the reviewer for their comments regarding the structure of the conclusions. We agree that it would be useful to refer to the research questions posed in the introduction and restructure per front relative region. This will be incorporated in the revised manuscript.

6 Writing style

This is only a minor point, but I would ask the authors to capitalize the first letter if a specific section or chapter is referenced (e.g. Section 1, instead of section 1). Moreover, please double-check the usage of ";" throughout the text (see also specific comments below).

Thank you for the comments on styling. According to the submission guidelines the acronym "Sect." should be used in text. We will pay careful attention to this when revising the manuscript.

2 Specific comments and technical corrections

1. l. 5 ”At other front relative regions”: At this point in the abstract, it is unclear what ”other front relative regions” refers to. Please rephrase or add additional explanations in the abstract.

   Thank you for this comment. We propose to change this to “Behind the surface front, cells form…..”. This conveys the meaning without having already read the rest of the manuscript.

2. **Introduction**

3. l. 15: ”; primarily due to convective permitting models (CPMs) at increased resolution”: This does not read like a complete sentence. I would suggest to avoid the usage of ”;” in the text (please also check later occurrences in the manuscript). I also think that it is common to use ”convection-permitting” (instead of ”convective permitting”).

We will change to “convection-permitting” as this is indeed more common and change the semi-colon to a comma for increased readability. Thank you for the suggestions.

4. l. 28: Please remove ”etc” and specify.

   Etc in this case indicates there are several other quantities that could be listed. However, since “such as” is already written before the list, the “etc” can be removed. Thank you for the suggestion.

5. l. 84: Please avoid double-brackets ”) (”. See also l. 129, 258, 357.

   We appreciate the suggestion, however there is no mention of double brackets in the submission guidelines so we will leave it up to the typesetters to decide on this styling aspect.

   https://www.weather-climate-dynamics.net/submission.html

6. l. 32 ff: This reads a bit colloquially, please rephrase.

   We suggest revising to “Anticipating the spatiotemporal onset of convective cells is essential for….”

7. l. 43 f: ”The literature would benefit from studies quantifying the relevance of frontal lifting at different regions relative to the front, especially during the warm-season.”: This sentence appears a bit out of context, I’m not sure if it necessary here.

   We will move this sentence to the end of the paragraph. Thank you for picking up on this.

8. l. 51 ff: The role of wind shear should be discussed together with other factors relevant for convection, i.e., in the paragraph starting in l. 27. Moreover,

following General Comment 1, I would ask the authors to include the background information provided in the Results sections in the introduction. Overall, the introduction would benefit from more clearly summarizing and structuring key variables relevant for convection, targeting specifically convection embedded in the frontal environment.

We will reorganise and streamline the introduction section as well as incorporate additional literature. Thank you for the suggestion.

9. **Methods**

10. l. 85: I would ask the authors to include one explanatory sentence on the TFP equation.

    We will add that "The term represents the rate of change of τ projected in the direction of the thermal gradient," Thank you for the suggestion.

11. l. 106: "The process is repeated 30 times to remove any local-scale features.": Please explain here, why specifically "30 times" was chosen. I would ask the authors to elaborate on why this specific smoothing method was applied (in contrast to other methods)?

    This was chosen subjectively based on looking at several case studies. There is no standard practice when smoothing as it depends on the resolution of the dataset. More smoothing reduces the strength of gradients. Smoothing more times while using a lower gradient threshold would yield similar results. Smoothing becomes particularly important when using convection-permitting models with higher resolution. We will note that this choice was arbitrary. Thank you for raising this point.

12. l. 109: "than some previous studies": Please include those studies here.

    We will provide Schemm et al. (2016) as an example who tested 300 km and 500 km front length criteria. Thank you for the suggestion.

13. l. 118 f: "This is also supported by the mean maximum climatological surface convergence in ERA5 data (Pacey et al., 2023; their Figure 3)". Please add some additional information on how this figure supports the statement, such that the reader does not have to read the mentioned publication themselves.

    The reader does not necessarily need to read the publication unless they would like to see the visualisation of this result. We will make this statement more explicit by saying "The mean maximum climatological surface convergence in ERA5 was found 300 km ahead of the 700 hPa front in Pacey et al. (2023) (their Figure 3), which supports the aforementioned assumption."

14. l. 132 ff. Please label the criteria following your approach above (i.e., "(A)", "(B)", etc.), and add text describing the criteria.

Thank you for the suggestion, we will incorporate this in the revised manuscript.

15. l. 142: Does the time of first detection correspond to the first exceedance of 46 dBZ? Would it be possible to track these cells also before they reach maturity, i.e. in their developing phase with lower reflectivity?

    Yes, it is the first exceedance. In principal, a lower threshold could be used to start tracking cells in their developing phase, but this information is not incorporated into the KONRAD2D dataset.

    l. 139 ff: "Since some cells have a lower area than the grid size the bounds are increased by 0.125 degrees (half a grid point)": I find this difficult to understand, please rephrase.

    We suggest to rephrase this to "To take the different spatial resolutions of the datasets into account and the fact that some cells are smaller than the ERA5 grid size, the cell boundaries are extended by 0.125 degrees in each direction"

16. Figure 1: This figure nicely illustrates the definition of the three categories. For further illustration, the authors could additionally include the position of the surface cold front as well as the defined pre-frontal, pre-surface frontal and post-frontal regions. *Thank you for the positive feedback regarding Figure 1. We will add a surface frontal line to Fig. 1a and if there is space it would also be nice to add pre-surface-frontal, pre-700-frontal post-700-frontal text labels. Thank you for the suggestion.*

17. l. 152, Table 2: I appreciate that detailed numbers of grid points in each category are provided in Table 2, yet it would be easier to comprehend if the numbers were shown as an additional figure.

    Thank you for the nice suggestion. This would indeed be more informative and match the style of other figures in the manuscript.

18. l. 161 ff: The choice of variables should clearly be motivated in the introduction, such that refering to literature in this short paragraph is not required anymore.

    See our response to general comment 1. We argue it is important to briefly introduce each variable, however we will make sure to substantially shorten such introductions. Thank you for the suggestion.

19. l. 166: As mentioned above, it is not fully clear to me why different definitions of CIN, but not of CAPE are used.

    This is primarily due to dataset availability, see our response to general comment 4 for further details.

20. l. 168: "So that a CIN value is present for all grid points": Please rephrase (e.g., "To ensure that ...").

    Thank you for the suggestion, we will revise the updated manuscript.

21. Equation 3: Please define the symbols/abbreviations in the text.

We appreciate symbols are typically defined in this way. However, since there is a rather long list of symbols to be defined, showing them as a column list increases readability. We thus remain with the original version.

22. l. 193 f. I would ask the authors to elaborate on the smoothing method and how the number of smoothing cycles was exactly determined (see also previous comment above)?

This is again subjective as it depends on the dataset resolution. In this case, we have already elaborated and noted that smoothing values between 10 and 100 were tested. We will add that the decision is ultimately subjective and that the same smoothing filter as used in section 2.1 is used. Thank you for this suggestion.

Results

23. l. 203: It could be helpful for the reader to show and discuss the occurrence frequency of convection (Fig. 12) before Sub-sections 3.1, 3.2, etc. to familiarize the reader with the dataset.

We thank the reviewer for the suggestion but since the discussion section, which will be renamed, serves the purpose of putting the results in the context of Pacey et al. (2023) we see it more fitting to include it in that section.

24. l. 207 ff: The authors emphasize the differences in dewpoint between convective and non-convective environments. Did the authors also consider the 2-m temperature distributions? Are the differences in dewpoint related to differences in humidity (i.e., dew point depression) or to differences in the background temperature? I would be interested to see Fig. 2 for 2-m temperature.

We show the surface temperature with the 25th and 75th quartiles shown as horizontal lines (Figure R3). A similar result is observed to the surface dewpoints with a significant positively anomaly at cell grid points at all front relative regions. The magnitude of the anomaly is very similar as well (around 3–4 ^{o}C). We will mention this result in the dew point section and include Figure R3 (except with median triangles) in the appendix.

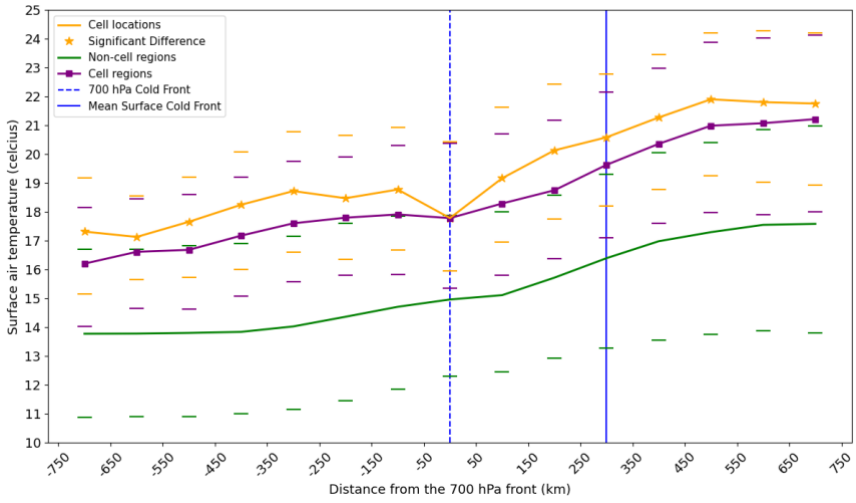

Figure R3: As Figure R1 but for surface air temperature.

25. l. 236-241. This is repetitive. Please streamline and/or move to Methods and Discussion Sections.

    We agree some of this can be shortened, however as mentioned in response to a previous comment we think it is useful to briefly introduce the variables in a few sentences. Thank you for the suggestion.

    l. 241 f: Please rephrase and avoid using ";".

    We will replace the semi-colon with a comma here. Thank you for the suggestion.

26. l. 259: I assume the two numbers have been swapped? I guess post-700frontal should have lower absolute CIN values.

    Thank you for picking up on this, we will amend this.

27. l. 262: "by forecasters" is not necessarily required.

    This is just to emphasise its usage in operational forecasting.

28. l. 263-271: Please streamline this paragraph.

    As mentioned in previous comments, we will do our best to streamline this paragraph and remove lengthy descriptions in the revised manuscript.

29. l. 279: "shift to the left on the plots": Please rephrase.

    We will rephrase this to "shift towards the left (cold side) with increasing height from 850 to 500 hPa."

30. l. 297 f: The authors show mean vertical velocity for cell grid points of on average a few cm/s. I would ask the authors to relate those numbers (in the Discussion) to typically observed convective updraft velocities and discuss its implications for using ERA5 vertical velocity for studying convection. Is vertical velocity normally distributed and are large outliers present?

    We thank the reviewer for the suggestion. The vertical velocities in ERA5 at convective cell grid points in our study are up to 2 orders of magnitude lower than what can be seen in both observations and numerical simulations of convective updrafts (e.g. Weisman and Klemp, 1982). Therefore, we are arguing there is some signal of partially resolving convection occurrence but not actual updrafts. We agree some brief discussion of this aspect could be useful and this will be incorporated in the revised manuscript.

31. Caption Fig. 7: Please correct the typo: ”Postive”

    Thank you for picking up on this, we will amend this.

32. l. 335-336: This sentence is not fully clear to me, please rephrase.

    We believe separating this into two sentences will increase the clarity. Thank for you the suggestion.

33. l. 351: I would appreciate if the authors could include a more original reference (in addition to EUMeTrain).

    We will add a reference to Browning (1990). Thank you for the suggestion.

34. l. 352: Please rephrase ”condensation” by ”cloud formation” or similar, as condensation inherently implies that precipitation only forms from warmphase cloud processes.

    We will amend this, thank you for the suggestion.

35. l. 354 ff: Fig. A2 suggests that a substantial number of identified cells occurs in an environment with large cloud cover, and thus, may be embedded in a larger precipitating cloud system (see Fig. 11), such as the warm conveyor belt (which has also been mentioned). The Discussion Section could profit from including studies on precipitation characteristics and distribution in the warm conveyor belt airstream compared to pre-frontal convection.

    Indeed, we believe that the majority of cells between the 700 hPa front and surface front are embedded in stratiform precipitation regions. We will include additional discussion on this in the revised manuscript and cite previous literature on such topics (e.g. Oertel et al. 2020). Thank you for the suggestion.

36. l. 361-364: I’m not sure if I agree with the conclusions in this paragraph. Assuming ERA5 would (at least partially) represent convection, the precipitation signal would show up in ”Large-scale precipitation”, and not in the parameterized convective precipitation. In this case, I would expect a difference between the three categories in Fig. 11a (which is very small, in particular pre-frontal). Instead, the parameterized ”convective precipitation”

differs between categories (Fig. 11b), suggesting that convection cannot explicitly be represented in ERA5 (and needs to be parameterized). Apologies, in case I mis-understood this paragraph. I would ask the authors to rephrase this paragraph.

I think this is indeed a misunderstanding. We don't argue that ERA5 can explicitly represent convection without parameterizations. Of course, convective precipitation can only appear if convection is triggered in the parameterization scheme. The triggered parameterised convection may feedback on the vertical velocity field due to condensation and latent heat release (and hence further ascent). Even with convection parameterizations though, there is no guarantee convection will be triggered in the correct place and time. We find that there is a significantly higher convective precipitation total where convective cells were observed compared to non-cell regions.

We will rephrase this paragraph mentioning some of the points above. Thank you for bringing this to our attention.

l. 366 ff: Please streamline this paragraph, this information has been repeated several times.

We suggest to explicitly state the purpose of this section which is to put the results in the context of Pacey et al. (2023). We will remove text focused on what was shown in earlier sections. Thank you for the suggestion.

37. l. 371 ff: I appreciate that the authors discuss and summarize the relevant factors for (i) pre-surface-frontal cells, (ii) near-700hPa-frontal cells, and (iii) post-700-frontal cells. While the overall structure is good, this section could profit from (even more thorougly) comparing the presented results to previous studies, and e.g., pick up literature that has been mentioned in the introduction.

We thank the reviewer for this suggestion. We will further review previous literature and add further citations where required.

38. l. 408: typo: include "◦" in "16 C".

Thank you for noticing this, this will be changed.

39. **Conclusions**

40. l. 401: Please remove "etc" and specify.

As the previous comment, this is used to indicate that there are several other examples which are not listed. As a previous comment, we suggest writing "such as" before the list and remove the "etc" at the end.

41. l. 408-426: In general, I think it is ok to use bullet points to emphasize key conclusions. Yet, I would reduce the number of bullet points and more strongly aggregate the relevant information.

We agree that some bullet points could be aggregated thus reducing the overall number of bullet points. Thank you for the suggestion.

42. l. 422 f: It is expected that convective cells are associated with positive vertical velocity. In Section 3.5, the authors have briefly mentioned the implication for consistently seeing this signal in ERA5 data. I think the manuscript would profit from a more thorough discussion on ERA5 and its ability to (at least partially) represent convection and convective precipitation. The last sentence of the manuscript brings up this open question, yet it could be discussed more thoroughly.

    To robustly assess how well ERA5 represents convective precipitation amounts would require observations of precipitation amounts. Here, we only use radar reflectivity which has additional uncertainties when converting it to a precipitation amount. Thus, based on our results we can only briefly speculate where this signal comes from. Therefore, we open this topic (and further discussion) for future work. As mentioned in our response to comment 36, we will further elaborate on what the convective parameterization scheme could mean for the vertical velocity field.

43. Have the authors considered including a schematic that illustrates and summarizes the relevant factors that discriminate between convective and non-convective conditions in different regions of the front?

    We thank the reviewer for this nice suggestion. We will work on designing such a graphic which could also serve as the highlight figure for the paper.

References

Browning, K, A,. 1990: Organization of clouds and precipitation in extratropical cyclones; in: Extratropical Cyclones, The Erik Palmen Memorial Volume, Ed. Chester Newton and Eero O Holopainen, p. 129 – 153

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